

800 pages, and when we consider the position which Prof. Kopp holds as an historian of chemistry, a position which demands that due regard should be paid to his writings.

The more immediate object of the pamphlet is, however, to reply to the strictures of MM. Bartoli and Stracciati on the law of molecular volumes. These gentlemen have criticised somewhat severely Kopp's work in this department without taking into consideration the obstacles which, at the time it was carried out (1855) stood in the way of accurate and definite investigation. Singularly enough, they do not escape the same charge, having themselves in some cases made use of materials of an insufficient degree of purity. It is likewise pointed out that they are labouring under a complete misapprehension of the views held by Kopp and the significance of his deductions, nor do they seem to have appreciated the difficulties that surround the establishment of a "physical law" of general application.

G. H. B.

LETTERS TO THE EDITOR

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[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

Organic Evolution

ALLUDING to the first instalment of your abstract of my recently published paper on "Physiological Selection," the Duke of Argyll remarks:—

"I rejoice that the author has at last discovered that 'natural selection has been made to pose as a theory of the origin of species, whereas in point of fact it is nothing of the kind.' This has been my contention for many years" (NATURE, vol. xxxiv. p. 335).

These words seem to imply that my views with regard to natural selection have now undergone an important change, and one which brings them into conformity with those that have for many years been contended for by the Duke. It therefore seems desirable to state that such is not the case; and as I can only attribute the misunderstanding of so able and friendly a reader to some ambiguity in the condensed abstract of my paper to which he refers, I may invite him to consult the paper itself, where the matter to which he alludes is more fully explained. He will there find that my views upon the subject of natural selection are the same now as they have been during the last fifteen years; that in all essential respects they still coincide with those that were held by Mr. Darwin; and that my "additional suggestion on the origin of species, although quite independent of natural selection, is in no way opposed to natural selection," but is to be regarded as indicating "a factor supplementary to natural selection."

The state of matters, then, is simply this. Mr. Darwin himself has freely acknowledged that his theory of natural selection is not in itself a sufficient explanation of the origin of species. He therefore supplemented the natural causes which are together comprised under this term¹ by sundry other causes of a similarly

¹ In common with many other critics of Mr. Darwin's work, the Duke of Argyll has always contended that the theory of natural selection is "fundamentally erroneous" in that it assumes "variations to arise by accident," and merely "groups under one form of words, highly charged with metaphor, an immense variety of causes, some purely mental, some purely vital, and others purely physical or mechanical." This, however, is no valid criticism of the theory, which for the first time did comprise under one general point of view all the causes which together go to produce the results. In the opinion of the Duke, the weakest element of the theory consists in its inability to explain the causes of those variations on the occurrence of which the theory depends (NATURE, loc. cit., p. 336). But it is clearly no valid objection to a theory which explains one set of causes that it is unable to explain another and ulterior set. So long as variations of all kinds are known to be matters of fact, they are available for the theory of natural selection, even though the ulterior or physiological causes of variation should never be discovered. And for all the purposes of this theory it makes no difference whether the variations which are seen to take place in all directions, *with and without respect to utility*, are spoken of as "accidental" or as due to hidden causes. All that this theory has to do is to take the principle of promiscuous variation in all directions as a datum supplied by observation, and from this fact to show how the further principles of heredity, struggle, and survival are

natural kind. Of these he attributed most importance to use, disuse, sexual selection, correlated variation, and prolonged exposure to uniform conditions of life. To these supplementary causes Moritz Wagner added independent variation in the absence of intercrossing with parent forms, while I have myself added physiological selection. Now, the whole body of Mr. Darwin's followers have agreed with him in holding that the theory of natural selection is not in itself a sufficient explanation of the origin of species. But many of these followers differ from Mr. Darwin, and also differ among themselves, as to the proportional part which the principle of natural selection is to be considered as having played in the evolution of species. Mr. Darwin thought that in this respect natural selection plays a more important part than any other principle [therefore it is hard to see how in this respect any of "the successors of Darwin" can possibly "have run quite wild from the teaching of their master"], while in the opinion of many of his followers this principle should be regarded as of a value subordinate to the others. Of all the writers who have taken the latter view, the most clear-headed, as well as the earliest and most persistent, is Mr. Herbert Spencer. He more than any other author has been instant, both in season and out of season, in giving reasons for the scepticism that is in him. I confess, therefore, to not understanding the Duke of Argyll when he says that in the two articles recently published by Mr. Spencer "we have for the first time an avowed and definite declaration against some of the leading ideas on which the Mechanical Philosophy depends." So far as I can see, these two articles convey little more than a reiteration of the characteristically Spencerian view that, in the course of organic evolution the processes of "direct equilibration" have been of more importance than those of "indirect equilibration." By the first of these terms Mr. Spencer means use, disuse, and all other causes tending directly to the production of adaptive structures, while by the second he means natural selection. Now, from the time when Mr. Darwin first published his "Origin of Species" the main point of difference between his views and those of Mr. Spencer has uniformly consisted in the estimates which they have formed of the relative importance of these two kinds of equilibration. Surely, therefore, this cannot be the respect in which it is said that Mr. Spencer has "now for the first time opened his eyes and his mouth." Yet, if not, I do not understand the allusion. The Duke seems to imagine that, in some way or another, Mr. Spencer has taken a new and important "point of departure" in the course of his speculative thinking, and one which is "fatal to the adequacy of the Mechanical Philosophy as any explanation of organic evolution." It would be a matter of great interest to me—and I am sure to many others who have read the articles in question—to be told in what respect Mr. Spencer has committed himself to so great a change of doctrine; and it would certainly be a matter of profound astonishment to all evolutionists if Mr. Spencer can be shown to have so much as insinuated that his "direct equilibration" differs from Mr. Darwin's natural selection in not belonging to the system of so-called Mechanical Philosophy—or that those factors of organic evolution on which he has mainly relied differ from those on which Mr. Darwin has mainly relied in lending better countenance to the Supernatural Philosophy of Design.

My own attitude with regard to all these questions is perfectly plain and simple. In common with Darwin, Spencer, and the great majority of evolutionists, I believe that in the origin and development of adaptations—whether structural or instinctive—two sets of strictly natural causes have been at work: I agree with Mr. Darwin in thinking that of these two sets of causes the "indirectly equilibrating" have been of more importance than the "directly equilibrating"; but I differ from other evolutionists, both of the Darwinian and Spencerian schools, in expressly drawing a marked distinction between causes of either kind which have been operative in the evolution of *adaptations* and those which have been operative in the evolution of *species*; and, lastly, I claim to have shown that when once this distinction is recognised, the theory of natural selection ceases to be, properly speaking, a theory of the origin of species; that it is thus liberated from all the difficulties with which it has hitherto been entangled on account of its having been made to "pose" as such; and that it is therefore placed in a position of greater competency to select the variations which happen to be useful from those which are not. One might as well object to the physical explanation of specific gravity in selecting or sorting the different materials of a sea-shore, on the ground that we do not know the causes either of gravity in general or of the variations that are observable among specific gravities in particular.

logical security than it has ever occupied before. Far, then, from joining the "contention" of my critic in seeking to depose natural selection as a theory of the genesis of adaptive structures and instincts, I have expressly sought to fortify that theory as a "mechanical" explanation of these phenomena. Whether or not I have been successful I must leave others to judge, *after they have done me the justice to read my original paper*. But, be this as it may, the ambiguity of that paper must indeed be extraordinary, if it leads any one to suppose that my argument is precisely the opposite of what it is intended to be.

Geanies, Ross-shire, August 16

GEORGE J. ROMANES

Meteorology and Colliery Explosions

THE catastrophe at Woodend Colliery on Friday last again directs attention to the connection generally believed to exist between explosions of fire-damp and atmospheric changes. The real nature of this connection is but little, if at all, understood. From time to time observations have been taken with the view of throwing some light on the subject, but as the observations in one mine were discussed without reference to what may have been occurring in other districts, the results have not added much to our knowledge. Meteorology, however, is now sufficiently advanced to permit the adoption of another and more satisfactory method of dealing with the question.

The idea so long prevalent that certain fixed points on the barometric scale indicated certain kinds of weather has now been discarded, the examination of synchronous weather charts showing conclusively that the weather changes are not so much dependent upon the height of the barometer at any one place as upon the relations existing between readings over a tract of country; the direction in which the highest and lowest readings lie, and the difference of pressure (the barometric gradient) between neighbouring places—these form the basis of our modern weather knowledge. Nothing of this kind has hitherto been attempted when dealing with observations from collieries. If the presence of gas in mines is in any way regulated by changes of atmospheric pressure, it would be well to see if, like the weather, it is distributed in areas, and whether in these areas again some localities would have more gas than others, dependent more upon the distribution of pressure than upon local readings. With the object of discovering some law or laws governing the presence of gas, I appeal to colliery officials in every coal-field in Great Britain and Ireland to supply me with the few observations detailed below for a period of four months—from September 1 to December 31, the best part of the year for such work.

The Meteorological Office Weather Charts issued daily (Sundays included) at 8 a.m. and 6 p.m. show the distribution of pressure, winds, temperature, and weather. To these I propose adding the information supplied from mines *at the same hours*. Those who cannot arrange for two observations daily, to give preference to the morning set. The gas observation being the most important, I would be glad if precise information can be given. Absolute uniformity cannot be expected, but I would suggest that, where possible, a disused gallery favourable to the object in view should be used, one where the changes in the quantity of gas can be stated in yards or feet, thus turning the gallery into a gas barometer somewhat similar to the one at Seaham Colliery after the explosion of a few years ago. Those who have not the facilities for such measurements can still give valuable data if they do no more than note the increase or decrease of gas as "slight," "moderate," or "great." The appended specimen of the form for recording the observations

Form for Recording Observations

..... Colliery, near

Date, September 1886	Top of shaft, feet above sea-level		In underground workings, yards from shaft feet below pit-bank			Gas in gallery, yards	Remarks
	Tempe- rature in the shade	Weather	Baro- meter	Tempe- rature	Quan- tity of air passing		
1st, 8 a.m.							
6 p.m.							
2nd, 8 a.m.							
6 p.m.							

has columns for the air-temperature in the shade at the pit-bank, and the weather; while underground, in addition to the gas-record, the reading of a barometer and thermometer, and quantity of air passing at a fixed point some distance in the workings; also, remarks on the variations of the gas and ventilation at other than the regular hours. A sheet of close-ruled foolscap, arranged as indicated, will contain the data for one month, and, as soon as filled up, should be forwarded to me at the Meteorological Office, London, S.W. On the back of the first return particulars are required of the geographical position of the mine, the name and address of the manager, whether the barometer is a mercurial or an aneroid, together with the readings of the same at the pit-bank at 8 a.m. daily for a week before taking it underground, and describing the plan adopted in measuring the gas. It must be understood that I am undertaking the discussion as my own work, for which the Meteorological Council is not responsible.

HV. HARRIES

August 16

Railway Weather Signals

WITH reference to the notice given in NATURE, vol. xxxiv. p. 347, of the ingenious plan adopted by the Norwegian Meteorological Institute for disseminating its weather reports, it may not be known to all of your readers that a similar system of signals has been in use for some time on the railroads in Ohio, Pennsylvania, and Canada. The day signals there consist of sheet-iron disks about three feet in diameter, and are displayed on the side of baggage-cars. The signals are shaped like the sun, a crescent, or a star, and differ in colour, being red or blue. The red colour refers to the temperature, and the blue to the state of the weather, as rainfall or snow. This system of signals was first brought into practical operation by Prof. T. C. Mendenhall, Chief of the Ohio Meteorological Bureau.

A system of night-signals for railways is also under trial in Pennsylvania: they are in the form of rockets or an exploding cartridge, which, when fired, may be seen from six to ten miles.

CHAS. HARDING

August 17

Tornaria and Actinotrocha of the British Coasts

THREE species of *Balanoglossus* are known to occur on the shores of North-West Europe. *Balanoglossus kuppferi* was taken by Willemoes Suhm at Helleback, in the Sound, that is, on the coast of Zealand (*Zeit. f. wiss. Zool.* vol. xxi. 1871); *Balanoglossus salmoneus*, Giard, and *B. rebinii* occur, according to Mr. Bateson's last paper in the *Quarterly Journal of Microscopical Science*, at Concarneau, in Finistère, and I believe also at the Channel Islands. But no *Balanoglossus* has yet been found on the shores of Britain. It will therefore be of some interest to British naturalists to learn that in August 1884 I obtained in the tow-net a larva which seemed to possess the distinctive characters of *Tornaria*. I had not leisure at the time to study the specimen with much attention, but I made a hurried sketch of it, which shows the presence of two parallel longitudinal bands of cilia anteriorly, and a single transverse band posteriorly. At the posterior end is a conical protuberance resembling the adhesive organ described by Bateson in his creeping larva. The position of the mouth was not ascertained, but was probably between the two anterior bands of cilia. The water vessel and tuft of cilia at the apex of the præoral lobe were not observed. This larva may not have been *Tornaria*, but I think it really was that form; and naturalists who are spending an autumn holiday at the seaside would probably, if they undertook the search, succeed in finding *Balanoglossus* in the littoral sands, and its larvæ in the shore waters.

Phorenis is also at present, I believe, excluded from the British littoral fauna, but is certainly present on our shores, though no adult specimens have been taken. I took large numbers of *Actinotrocha* in the tow-net, close to the shore, in September 1883, a little to the north of the mouth of the Cromarty Firth. If I am mistaken in supposing that adult *Phorenis* and *Balanoglossus* have never been found on the coast of Britain, I shall be glad to be corrected.

J. T. CUNNINGHAM

Mock Suns

As I observe the omission from my letter on the parhelia or mock suns of last month (p. 313) of the diagram which was